California Climatic and Environmental History and impacts on local San Francisco Bay−Delta Wetland Habitats

#0046

Technical Panel Review

Proposal Name: California Climatic and Environmental History and impacts on local San Francisco Bay−Delta Wetland Habitats

Applicant Organization: Regents of the University of California

Principal Lead Investigator(s):
Ingram, B. Lynn

Amount Requested: $305,481

TSP Panel Summary of Findings:

The researchers proposing this work are ideally suited to carrying out the work proposed. However,there are two troubling issues with this proposal that warrant serious consideration. The first issue is whether the results can meet the study objectives – "…resolve the environmental history of climate variability in the San Francisco Bay−Delta watershed and habitat responses to changing conditions within the Bay−Delta." The critical external reviewer does not believe that reconstruction from four tidal marsh and two aquatic core samples from Suisun will do this job. The researchers do a good job of placing the work into context of the reasonable sized body of prior work, so one question is whether these six cores are the final lynch pin to complete the story or whether they merely add to its completion and thus the researchers need to provide appropriate project goals. The second issue is the effort to reconstruct sediment sources. It will inform past central valley watershed responses to climate change, but the modern world of dams and water diversions mean that any future sediment response will bear little resemblance to the past. Thus, the informational gain from $67,000 is low for CALFED. Getting cores from more locations would make the greatest contribution. Site choices are few but there may be enough to prove valuable (in−channel historic marsh such as Mandeville).

The panel has the following recommendations: 1) It would be especially valuable to collect some cores from a natural Delta

freshwater tidal marsh to see how the Delta has responded historically. 2) Take out the section of proposal re: sediment origins. It may be better to spend that money on additional core samples (see #1 above) Exact sites should be worked out ahead of time and approved by CALFED. 3) Proposal needs to be clearer in terms of how many core samples are to be taken and especially analyzed – 22? 6? It is unclear from what is budgeted. 4) Rework budget to allow more core samples and take into account receipt of NSF funding (or not). Can additional funding be obtained if NSF doesn't fund?

Relevance to PSP Topic Areas:

Moderate

TSP Technical Rating:
Sufficient

TSP Funding Recommendation:
Do Not Fund

TSP Amount Recommended: $**0**

Conditions:

External Technical Review #1

Proposal Title: California Climatic and Environmental History and impacts on local San Francisco Bay−Delta Wetland Habitats

Proposal Number: 0046

Proposal Applicant: Regents of the University of California

Purpose

Comments	**The goals and objectives are clearly stated, but there are no hypotheses presented for testing. The idea of combining these techniques in sedimentology, elemental analyses, paleontology, palynology, isotope and radiochemistry to address paleo−environmental reconstructions are not new. Work of this type is important, but examining change over this time scale (last 6,000 years) is less relevant to CA Bay−Delta Authority primary objectives than examining change over the last 500−1000 years at a larger number of sites. The amount of related work described as recent and ongoing, in addition to the small number of cores (6 of 22) that will be described from this funding, indicates that the work may not be justified. Selection of a research project classification for this work is appropriate. The results are likely to add to the base of knowledge, but 300k to work up 6 cores does not seem cost effective. The project is not likely to generate novel information, methodological advances or new approaches.**
Rating	**Sufficient**

Background

Comments	**There is a conceptual model presented, but it does not effectively or persuasively explain or substantiate the need for this work. The PI presents a summary of an extensive body of work that has already been done on this system, addressing very similar and**

	overlapping objectives. The main thrust of the justification for applying this conceptual model are gaps and inconsistencies in the paleo−environmental reconstructions assembled to date. An issue that hardly seems likely to be solved with 6 sediment cores. The discussion of using sediment elemental composition as a fingerprinting techniques to determine sediment sources to the estuary is interesting, but exactly how that will be related directly to in−phase/out−of−phase hydrologic regimes between the two major rivers and also to atmospheric processes is not well articulated. The basis for the proposed work is outlined, but a more direct connection between the proposed research and the priority objectives of CALFED needs included.
Rating	**Inadequate**

Approach

Comments	**The approach is not well designed and appropriate for meeting the objectives of the project in terms of the number of cores that will ultimately be worked up. The approach in terms of what analyses will be carried out on those cores is well designed, interdisciplinary and comprehensive. It is clear who will be performing management tasks and administration of the project and resources are set aside to do so, but the distribution of those resources seem less than optimal (see budget discussion). Scientific products of value are likely from the project, if the 6 cores worked up here are included in a much larger and more comprehensive data set, but not likely to be of significant scientific value as a stand−alone product. There is a plan for dissemination of information gained from the project (community college and professional meetings presentations, website data postings, publications), but this is standard outreach rhetoric that sites no specifics (what community colleges?, have you made contacts for outreach and with who?; what about K−12 involvement?; how often will these outreach efforts be made over the course of the project?; etc.).**

	Contributions to larger data management systems are relevant and are considered by the PI.
Rating	Inadequate

Feasibility

Comments	The approach is not fully documented, as previously discussed in that section, but most of what is described is technically feasible. Again, the likelihood of success as a stand alone product is low, but as part of a much larger, comprehensive work, it is likely that this work would make some important contributions. The research described is within the grasp of the assembled researchers but the success of the work is contingent on a number of factors, including NSF funding for some of the core collection and the ability to integrate this work into a larger data set.
Rating	Inadequate

Budget

Comments	Yes, it is clear how much each aspect of the proposed work will cost including each task and salaries. Support for equipment is not requested. The budget is reasonable and adequate for the work proposed, but the proposed work, as previously discussed, is too narrow to accomplish the objectives as a stand alone product. Additionally, asking for 300k to process 6 sediment cores is simply not reasonable. A closer look at the budget makes clear that the primary expense is supporting three years of salary for the PI's post doc (155k+), not doing comprehensive sampling and science.
Rating	Inadequate

Relevance To CALFED

Comments	This proposal only addresses one of the CALFED identified priority topics; #4 habitat availability and response to change. The proposal does do a nice

	job of integrating some of the other priorities listed in the PSP, including integration with previous and ongoing research, use of existing information, having a strong interdisciplinary framework and a modeling component. If the authors are able to integrate this work successfully with other completed and ongoing work in this system, it will likely yield information useful to CALFED resource managers and policy makers.
Rating	Sufficient

Qualifications

Comments	The authors and affiliated researchers have an above average record of past performance. There is little doubt that the project team is qualified to efficiently and effectively implement the proposed project. As evidenced by cooperative letters of support, the authors do have available the infrastructure and other aspects of support necessary to accomplish the project.
Rating	Above Average

Overall Evaluation Summary Rating

Comments	The authors seek to reconstruct several elements of the climatic history of a large and complex region of California, the San Francisco Bay–Delta system, over the last 6,000 years. They describe a series of multi–disciplinary techniques they wish to employ to accomplish this objective, including methods in sedimentology, sediment geochemistry, radiochemistry, stable isotope geochemistry and paleontology. The authors plan to integrate data collected in this effort with an existing and growing database of the region. Despite the qualifications, affiliations and reputations of the assembled research team, the proposal

	suffers from a number of shortcomings that result in the listed rating; these include: **1. Reliance on funding from other agencies to accomplish sample collection (NSF). 2. Issues of scale. The work as proposed is insufficient as a stand alone product and compatibility with the larger data set has not been thoroughly addressed, six cores does not a paleo−reconstruction on this scale make. 3. Budget. The authors are asking for 300k+ to process six cores, which is simply not reasonable. More than half of the budget is to used to support three years of a post doc's salary. A reduction and redistribution of these funds would have resulted in a much more comprehensive sampling scheme and stronger proposal.**
Rating	**Inadequate**

External Technical Review #2

Proposal Title: California Climatic and Environmental History and impacts on local San Francisco Bay−Delta Wetland Habitats

Proposal Number: 0046

Proposal Applicant: Regents of the University of California

Purpose

Comments	**Yes, to all of the above. The team proposes to collect a total of 22 sediment cores at 11 sites in and near Suisun Bay, and extract information by several methods. The goals are clearly stated, and the project is very timely in view of its relevance to climate change/sea level rise impacts. Understanding past environments in the Bay−Delta system is key to understanding and planning for the future.**
Rating	**Superior**

Background

Comments	**Yes, the background and conceptual models are clearly stated and explained in the context of previous work, and of the expected benefits of this project.**
Rating	**Superior**

Approach

Comments	**The approach is very well designed. The methods that will be used are up−to−date and require top−quality lab facilites, but the team is experienced with the types of analyses proposed. The administrative structure is relatively simple, since the scientists will be doing most of the work, with help from lab assistants. The proposers have a good track record of publishing results and presenting at conferences, including the CAFFED science conferences. The data**

	will be made publically available on a web site as well as through publications.
Rating	Superior

Feasibility

Comments	The approach and methods are fully-documented. and the likelihood of success is very high, based on the experience and record of these scientists. The scale is realistic, consistent with the objectives, and clearly within the grasp of the authors.
Rating	Superior

Budget

Comments	The budget is detailed,realistic and well-thought-out. I note that a separate proposal has been submitted to NSF for core collection and curation, increasing the "leverage" of this proposal.
Rating	Superior

Relevance To CALFED

Comments	The proposal directly addresses CALFED-Science Topic 4: Habitat Availability and Response to Change. It is also integrative and multidisciplinary, since reconstructing paleo-environments from sediment cores requires integrating knowledge from plant and animal ecology, geochemistry, sediment stratigraphy, estuarine hydrodynamics, etc. It will build on previous research, but may fill important gaps in our knowledge of the history of salinity, temperature and sediment regimes in the Delta and Suisun Bay. The information will be useful to CALFED resource managers and policy makers if they have a long-term vision and perspective.
Rating	Superior

Qualifications

Comments	**Drs. Ingram and Malamud-Roam have excellent research and publication records, include a track record of successful CALFED-funded research. They are eminently qualified, and have access to the necessary lab and field resources to pull this off.**
Rating	**Superior**

Overall Evaluation Summary Rating

Comments	**This is exactly the kind of research that CALFED should be supporting. It is on the "cutting edge", and is directly relevant to understanding and predicting the massive changes in the Bay-Delta system that are coming, even with an agressive program to control global greenhouse gases.** **I look forward to seeing the results of this research.**
Rating	**Superior**

External Technical Review #3

Proposal Title: California Climatic and Environmental History and impacts on local San Francisco Bay−Delta Wetland Habitats

Proposal Number: 0046

Proposal Applicant: Regents of the University of California

Purpose

Comments	**The research proposed will provide a history of salinity, water temperature, relative sediment inputs from the Sacramento and San Joaquin Rivers, large scale hydrological events and earthquakes and tidal marsh vegetation response over the last ~6,000 years. After reading the Science PSP I assume that this information is timely and important to CALFED and any modeling efforts, from those that address invasive species to problems such as sea level rise. This is an extension of previous research. Through this proposal investigators will improve spatial resolution, locally and regionally, to fill gaps, for example, to explain seeming contradictions in salinity fluctuations and tidal marsh vegetation response that occurred ~800 and 1200 years ago. They will also add a "layer" to their historical model – relative contributions of sediments from the Sacramento and San Joaquin Rivers in response to past precipitation regimes (already available from dendroecological records). These results are highly likely to add to "the base of knowledge" and provide novel information. The gaps identified may be simply due to a lack of adequate spatial resolution, but in filling in this last bit of the paleohydrologic/environmental history, investigators are likely to reevaluate parts of their model, thus processes and relationships in the climate−watersheds−estuary−marsh/sea level system they are documenting. New critical linkages may be revealed. This is a full−scale implementation project,**

	based upon previous research in the area.
Rating	Superior

Background

Comments	The conceptual model and underlying basis are clearly stated. A number of details are documented in the proposal and others are available in publications of the investigators (which I have consulted).
Rating	Superior

Approach

Comments	The approach is very comprehensive. Investigators will be examining a complex suite of paleoindicators that provide info on the history of the hydrology of the San Francisco Bay watershed and estuary. This info is essential to understand the range of past variability in the system and how it is likely to respond to future perturbations, natural or anthropogenic. Investigators have an established record of dissemination of research results locally and internationally, and plan to continue with the products of the proposed research.
Rating	Superior

Feasibility

Comments	Most techniques used in the research proposed have already been conducted by the investigators – and published. These publications have demonstrated the feasibility of the work in San Francisco Bay. Collaborators are applying methods that are well-tested, or in the case of testate amoebae analyses, show great promise (the success of the proposal does not depend on this analysis).
Rating	Superior

Budget

Comments	**The budget is vague and lacks specifics on costs, but this may be how the request for proposals was structured. Task 5, for instance, includes analyses conducted by other labs, yet details are lacking on cost per sample, how much is for labor, amount needed for lead−210 vs. radiocarbon dating, etc.. The $47,127 allotted to this task for dating, testate amoebae and magnetics analyses is more than adequate for dating of the number of samples identified. However, it is not clear what funds, if any will be transferred to Goman for testate amoebae or Verosub for paleomagnetic analysis. I would extend a similar assessment to other tasks. Funds are adequate to complete the work described. Funds for core collection are sought elsewhere, from the US National Science Foundation (NSF), It is not clear how much has been requested from NSF, as applicants state "are expected to cost $64,000." I have trouble with the rating system suggested for an item such as budget – it is either adequate and appropriate or inappropriate (not enough or too much). Considering the large number of labor− and instrumentation−intensive and analyses incorporated in this research program, I would deem the budget appropriate, thus my choice of "Sufficient."**
Rating	**Sufficient**

Relevance To CALFED

Comments	**This proposal directly addresses Topics 1, 3 and 4. By providing basic info on the history,**

	nature and drivers of hydrologic regime, temperature, and salinity of the estuary, it provides critical background for Topic 2, as well. By providing a history of paleohydrology, water salinity, temperature while being able to detect system and marsh response to extreme events, questions as to processes and future responses can be answered. It is essential to understand natural variability and range of response to develop models of future responses. As the applicants have pointed out – conditions in the recent century may not be representative of the past. Previous conditions may provide excellent models of driver/response relationships of the hydrological systems.
Rating	**Superior**

Qualifications

Comments	**The principal investigator, Dr. Ingram has a proven track record in conducting the type of research proposed and Dr. Malamud−Roam, whose time will be dedicated to the project, has proven skills in techniques of paleoecological research (palynology and macrofossils), experience in use of radiocarbon dating and much of the geochemical analyses. Collaborators all have abilities proven through publication.**
Rating	**Superior**

Overall Evaluation Summary Rating

Comments	**The only flaw I find is a lack of budget details – otherwise the project is exceptional in its inclusiveness, application of tested methodologies, and qualifications of scientists conducting the research. I see little risk in this project, but considerable added value to work already completed. This research program will provide a paleohydrological context to inform nearly all the issues identified in**

	the PSP. It will do something much more than asked for in any one topic or question listed in the PSP.
Rating	**Superior**